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My discussion of my subject has been brief, though, perhaps, as long as your desire. I have tried to show you that the wide influence of the engineering schools is of two branches: First, a direct effect exerted through the graduates extending the useful applications of science to the advantage of man (which is the effort of every true engineer); second, an indirect (but equally important) effect resulting from the admirable education disseminated amongst the people. And I have pointed out not only elements of great educational strength, but also some sources of weakness in the schools. It has been my particular wish to bring to your mind some image of the potent influence for good which has been in the past, and still more may be in the future, borne on the body politic by these schools, and to impress you with the desirability of bringing to their support the same bountiful endowments that are now justly flowing to the support of the medical schools. I trust that I may have interested you and that I may have reached, in some degree at least, my object.

In the course of my remarks I have had frequent occasion to use the phrase 'applied science.' You must not mistake me. Applied science is not something set off by itself and differing from 'pure science,' so-called. Far from it. It is pure science, if you wish, pursued in the stimulating, nutrient atmosphere bred of the belief that all scientific knowledge returns to its possessor great good in proportion to the advantages which he, through it, brings to mankind. Such an atmosphere is to be found in many of our medical schools and, I hope, equally in our engineering schools.

DUGALD C. JACKSON.

UNIVERSITY OF WISCONSIN.

*STAMENS AND PISTILS ARE SEXUAL ORGANS.**

THE statement in the above title will be received by some of my hearers with wonder that so obvious a matter should need any discussion, while by others, especially those versed in the modern morphology, it will be met by emphatic dissent. Yet I am convinced of its truth, and venture here to rise in its defense.

The discussion of the subject is not new. Professor L. H. Bailey, in *SCIENCE* for June 5, 1896 (reprinted in his 'Survival of the Unlike,' page 67), defended, with his usual clearness and vigor, the application of the sex-terminology to stamens and pistils; and he was answered in the same journal for June 26 by Professor Barnes, who maintained the strictly morphological view that the sex-terminology should be restricted to the gametophytes, or so-called sexual generations, within the pollen grain and the embryo sac of the ovule. Recently this morphological view has again been emphasized by Professor Ramaley, in *SCIENCE* for June 20, 1902, and he puts the case in its extreme logical form when he says: "The stamens, therefore, can not be male organs, nor the carpels female organs. * * * There are no such things as male and female flowers, nor flowers which are unisexual or hermaphrodite." This view I hold to be an error, for the reasons which follow.

To prevent misunderstanding it should be said at the outset that there is no difference of opinion as to the morphological facts involved. We all agree that the contents of the embryo sac when it is ready for fertilization, and of the pollen grain when in the corresponding condition, are the gametophytes, the precise morphological equivalents of the prothallus or sexual

* Read before the Society for Plant Morphology and Physiology at the Washington Meeting, December 30, 1902.

generation (gametophyte) in the pteridophytes. Where I differ from the extreme morphological view is just here, that while I admit that all sexuality, in whatsoever that may consist, is confined to the gametophyte in the lower forms where the two generations (as best manifested in the ferns) are structurally, morphologically and physiologically distinct, I deny that sexuality is confined to the gametophyte in the higher plants, where the gametophyte has become structurally incorporated with, and physiologically dependent upon, the sporophyte. If, then, sexuality is not confined to the gametophyte of the flowering plant in fact, obviously it should not be in terminology.

We must here note an important point in the discussion, namely, that it has two distinct phases: (1) There is the matter on which Professor Bailey argues, that, as a matter of propriety in usage, the old and familiar sex-terminology should not be wrested from its prior and consistent analogical significance and given a new and technically limited morphological application. (2) There is the new contention here defended, that a restriction of the sex-terminology to the gametophyte in the flowering plant is incorrect in fact. We may best consider them separately.

As to the first, and allowing for the moment (for clearness of argument) that sexuality may be confined to the gametophytes in the flowering plant, I think Professor Bailey's argument for the retention of the sex terminology to its present application is perfectly conclusive. He is certainly correct in his contention that the original sex-terminology was based upon analogies, with no thought of homologies; a male organ was that structure which secured the formation and functioning of the male element, and such an organ a stamen is; a female organ was that struc-

ture which secured the formation and functioning of the female element, and such an organ a pistil is. Now morphologists have no right, I believe, to attempt to wrest the sex-terminology from its consistent, intelligible, widely-used and *prior* application to analogies, and give to it a new and technical use for homologies, an attempt made still less excusable through the claim of its advocates that the earlier application is erroneous and only theirs is correct! Science is expected to apply new terms to its discoveries, and new conceptions; it should not attempt to appropriate an older terminology to new uses. As a matter of fact science *has* given an ample terminology of its own to the parts of the plant involved in the present discussion, and the confusion which has arisen in teaching and elsewhere is the result of a neglect to make full use of those terms, a neglect due no doubt to the mistaken notion that an adaptation of the older terminology to the new conceptions would conduce to clearness. I am of opinion, based upon some experience, that the difficulties in teaching, of which Professor Barnes and Professor Ramaley speak, can be met by a rigid application of the definite scientific terms *sporophyte* and *gametophyte*, with an abandonment of the misleading terms *sexual* and *non-sexual generations*.

We consider next the second point, whether, as a matter of fact, sexuality is confined to the gametophyte in the flowering plant. At the one extreme is the gametophyte of the fern, independent anatomically, morphologically and physiologically from the sporophyte; to it the name sexual generation (viz., that generation which produces the sexual elements) correctly and appropriately applies. At the other extreme is the gametophyte of the specialized phanerogam, where the gametophyte is formed, nourished and de-

veloped entirely within the tissue of the sporophyte, in the most intimate anatomical and physiological contact and dependence upon the latter, and is quite incapable of developing the sex cells without the direct cooperation of the sporophyte. It is plain that a part at least of the work of nourishing and preparing the sex cells for their functions, assumed by the prothallus in the fern, has in the phanerogam been transferred from the rudimentary prothallium to the highly developed sporophyte. The morphological line between gametophyte and sporophyte can still be traced (though only through recondite comparative researches), but the physiological, and to a great extent the structural, line between the two has vanished. The gametophyte, therefore, does not constitute a 'generation' in the sense in which the word was originally used in the ferns, for the physiological equivalent of the sexual generation of the ferns, is, in the phanerogam, the gametophyte plus part of the sporophyte.* Not only are the tissues of the sporophyte in the immediate vicinity of the gametophyte specialized to aid the latter in its work of developing the sex cells, but this is true (though to a lesser extent) of the sporophyte tissue for long distances away, even to the confines of the parts we call stamens and pistils, so that I can not doubt that some at least of the attributes properly belonging to a 'sexual generation' have been transferred that far back from the gametophyte into the sporophyte. It is no objection to this view that

* The intimate physiological interlocking of gametophyte and sporophyte is strikingly illustrated in the phenomena of polyembryony, where the sporophyte (nucellus) has acquired the power of producing embryos within the embryo sac, which embryos, although purely asexual, have the general form and course of development of embryos produced by the gametophyte. The physiological equivalence of perisperm and endosperm points in the same direction.

I can not tell where in the ascending series the 'sexuality' begins to pass over to the sporophyte; even if we knew precisely the actual stages in the evolution from fern to phanerogam (which we do not), and even if we were agreed upon a definition of sexuality (which we are not), it might still be impossible to tell precisely, so subtle are the gradations of natural processes, and so regardless are they of definable categories.

The sum of my argument, then, is this—that in the phanerogams the physiological line between the two 'generations' has vanished, and that a large part of the original function and attributes of the gametophyte has been transferred to the sporophyte which has had its tissues specialized to that end; hence the gametophyte of the phanerogams is no longer a 'sexual generation' in point of physiological fact, and it is misleading to use the name as an expression of morphological relations; sex not being confined to the gametophyte, the sex terminology can not be.

Only the morphological line remains to mark off the two generations in the phanerogam, but it is precisely this fact which has caused the whole difficulty. Morphologists have found so great a satisfaction in tracing the intricate but beautiful homologies from fern to phanerogam, that their attention has become centered exclusively upon the morphological phases of the subject, to the exclusion of its physiological aspects. They have forgotten that sexuality is more a matter of physiology than of morphology, and that function cuts across morphological boundaries in the most irrelevant manner. They have fallen into that error, against which Goebel has so forcibly warned us, of attempting to interpret morphology without reference to function, a method which can lead only to

a sterile formalism quite unrepresentative of nature's unconventional methods. That they have in this case fallen into this pit is due, I think, to the misleading influence of words. Starting with forms in which there are two distinct generations (as in the ferns), and applying very appropriately the terms *sexual generation* to the gametophyte and *non-sexual* to the sporophyte, they have kept these names for the morphologically equivalent stages in the evolution to the phanerogam, not noticing the gradual emptying of the names of their original physiological significance; until, finally, the names themselves have come to stand in their minds for the facts they state, and to be accepted as evidence, or even as final authority, upon the points at issue. The mischievous terms *sexual* and *non-sexual generations* have been and are the cause of the whole difficulty. Let us abandon them.

W. F. GANONG.

SMITH COLLEGE.

A TROPICAL MARINE LABORATORY FOR RESEARCH?

DESPITE the creditable activity which has developed in our country in biological research during the past few years, it must be confessed that it is difficult to explain the neglect upon the part of our naturalists to avail themselves of the opportunity to study the marine life of the tropical Atlantic, especially as one of the most, if not the most, favorable locality for the prosecution of such researches lies within our own territory at the Tortugas, Florida.

As Professor Davenport aptly states, we know more of the life of the Red Sea than we do of that of the Caribbean and Gulf of Mexico.

Our knowledge of the life of the tropical Atlantic is almost wholly dependent upon the results of brief and cursory expeditions, and the innumerable researches which re-

quire a permanent station for their successful prosecution have hardly been attempted. The mere systematic study and classification of forms in our tropical waters is glaringly incomplete, while we have almost failed to take advantage of the exceptional facilities which a tropical station offers for physiological and embryological studies, owing to the fact that the water in the tropics may be readily maintained at the same or at even a lower temperature than that of the ocean itself. In consequence of this and of the remarkable purity of the ocean water at the Tortugas and Bahamas, it is possible to rear larvæ or carry out physiological experiments with far better success than is attainable in our northern stations. If much has been accomplished in work upon the limited fauna of the southern New England or Carolina coasts, how much more might be expected from a study of the far richer fauna under the more favorable conditions attainable in the tropical Atlantic.

The cause of this neglect has been that none of our educational institutions has been able to afford to maintain a permanent laboratory in the tropics, and no co-operation has yet been, or is likely to be, effected which could bring such a laboratory into being.

The establishment of the Carnegie Institution has suddenly changed the aspect of the case, and as it appears to be the province of this institution to support important research work which none of our existing institutions has been able to afford, the prospect for the establishment of a permanent research laboratory in the tropical Atlantic appears for the first time possible.

As far as the writer is aware, no application for the establishment of such a laboratory has yet been addressed to the